apparatus required for dark-ground illumination. No one who works with serial slides or large blood films can put up with the small amount of motion which suffices for examining a minute drop of fluid. The examination of large culture plates is also difficult if the mechanical stage is not removable. Mr. Barnard has pointed out that the adjustable object-glass holders cannot, and are not intended to, replace the centring adjustment of the substage condenser. Such a suggestion surely originated from one who had never tried to centre his light in this manner. The centring of the substage condenser is considered by many at the present time to be an unnecessary refinement, and for those who are content to use such a badly corrected appliance as the so-called Abbe condenser it may be so. It is to be hoped that the proper use of a well-corrected achromatic condenser will be more widely appreciated in the near future, and we shall hear less criticism of the so-called dilletanti to whose labours the perfection of the instrument is largely due.

Amongst the minor points that have been raised, I noted with interest the suggestion that the mirror should be fixed in the optic axis. No doubt the mirror is seldom, if ever, used out of the axis, but it should not be overlooked that to be able to swing it entirely to one side for changing the apparatus in the substage or for taking the illumination direct from the source of light is a great convenience. It is rather surprising to see such stress laid upon a large body-tube as preventing internal reflections. I know of no optical instrument in which a large tube is supplied for this purpose, for the very good reason that the reflections are much more efficiently stopped by a series of diaphragms in the tube. There is no virtue in a large body-tube when it has within it a small draw-tube extending almost its whole length. The object of the large tube is to enable low-power photographic lenses to be placed in it when wide-angled views of large objects are required, the draw-tube at one end and the nosepiece at the other being removed for the purpose.

As an English manufacturer I am not prepared to discuss the opinion that is said to exist in some quarters that Continental instruments are more suitable to scientific needs than those made in our own country. I would merely ask that if any hold this opinion they will patriotically assist the industry by letting the British maker have an exact specification of their requirements, and I believe they will find us at least as adaptable as any in meeting their wants. CONRAD BECK.

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I HAVE read with much interest the letter from Mr. Barnard on the above subject, which appeared in NATURE of January 25. Being the writer of the third section of the article which appeared in NATURE of December 21, I should like to reply to several points raised by Mr. Barnard contradicting some of my statements.

In the first place, it must be mentioned that Mr. Barnard takes certain statements as inferring far more than they are meant to, or do, imply.—There is nothing in my statements to imply "that the present-day English microscope is a degenerate form of what was originally a complicated and massive piece of mechanism." I stated it was a *simplified* form of its predecessors—a virtue indeed, therefore an advantage over the old English type.

I still maintain that the Continental instrument, having evolved from an exceedingly simple design, is a more highly satisfactory instrument. It is obvious that Continental makers have copied many ideas from the English, but it is equally obvious that the English makers have copied the Continental in many respects. If we compare the current catalogues of the principal makers in all countries where microscopes are constructed, it would be exceedingly difficult to define nationality in regard to design. Perhaps the least said respecting the country of origin the less likely one is to offend possible feelings of patriotism. The subject before us, however, is the question as to whether the English or the Continental microscopes are the superior, and in the article referred to I have dealt with the instruments as they are generally produced at the present day.

As regards the base, it is generally admitted that the horseshoe form is more convenient for the average worker than is the tripod, mainly on account of his having to employ the instrument more frequently in the vertical position; therefore a well-designed horseshoe, or modified horseshoe, as is generally found in the leading Continental stands, serves the purpose equally as well as the tripod. Only when the instrument is placed in the horizontal position does the tripod prove superior as regards rigidity.

The microscope is very rarely used horizontally, excepting in photomicrography, a work which does not concern the average microscopist; consequently, the greater rigidity in the tripod base is of no practical advantage. In other words, it is similar to making a 40-foot ladder to reach a height of 20 feet. Even when the microscope is considered for photomicrography, there is no practical advantage in the tripod over the horseshoe. (In the term horseshoe, I wish it to be understood that the models which Mr. Barnard mentions as having no rigidity or firmness in any position are excluded from the argument.) If the tripod base is not screwed or clamped to the photomicrographic apparatus, or held in position by other means, it cannot possibly remain in proper alignment for any length of time. Having occasion to use both the horseshoe and the tripod base in photomicrography, I fail to find any difference in respect to rigidity when the stand is securely fixed in position. When vibration was purposely set up I failed to observe it less in evidence in the microscope with the tripod base.

In the case of the tripod, I have it securely fixed in position by three carefully fitted recesses in the base-plate of the photomicrographic apparatus, and when the microscope is removed for visual work it can be easily and quickly returned to position for a photographic record to be made. On the other hand, the horseshoe base is securely clamped down on the base-plate, and thereby the two pieces of metal form, as it were, a solid mass. Provision is also made for quickly and accurately replacing the horseshoe upon the photomicrographic apparatus.

If the instrument built upon a horseshoe base was constructed of light or springy material, I could understand Mr. Barnard's contention that "the instrument is under considerable strain and tension."

In regard to the substage and the question of centring condenser *versus* centring objectives, Mr. Barnard's remarks amount to an admission as to the imperfect construction of the instruments he has employed. Such remarks would cause a careless or otherwise incapable mechanician to heave a sigh of relief in the expectation of the inferior workmanship that would evidently be accepted. The statement refuted by Mr. Barnard is that greater accuracy is obtained, especially in photomicrography, by having centring screws controlling the objectives instead of the substage condenser.

I cannot look upon the substage condenser of a microscope as being "an independent optical system carried on a separate part of the instrument and extremely difficult to ensure accurate alignment," but rather that it forms part of the system. In the best Continental instruments the dove-tail groove carrying the substage is formed out of the solid metal which constitutes the limb; that is to say, the limb of the microscope supporting the body-tube, coarse and fine adjustments, is carried down below the stage, and also supports the stage and substage fittings, therefore, is made out of a solid piece of brass giving the utmost strength and rigidity. When it is possible for the mechanician to provide accurate alignment in the body-tube for the highest power objectives, surely, then, it is less difficult to provide substage mechanism to carry a condenser, "even of the finest construction, which does not focus within such narrow limits." Continental microscopes are provided with substage mechanism in accurate alignment, and maintain alignment equally as well as the mechanism carrying the body-tube. Considering that condensers do not focus within such narrow limits as objectives, it is a less difficult task to adapt a series of condensers to the optic axis. Consequently, the substage condenser and the eyepiece become two fixed points in the optic axis of the microscope. As the leading makers provide objectives so well centred that the differences are but slight, the adjustments in the objective sliders effect all that is necessary to obtain the most accurate centration,

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and with a considerable saving of time and trouble. Once the position of the substage condenser is altered the whole system of illumination from the substage condenser to the source of light is displaced, which must again be adjusted, so that we have to effect centring at various points on the optical bench. This is entirely avoided by having the substage condenser supported in a fixed sleeve, and the centring screws attached to the objectives or the nose-piece.

As regards the draw-tube, the point is that the use of the mechanical draw-tube is advocated more in England than are the correction collars, but *vice versâ* on the Continent. When one is using an objective without correction collar, which is extremely sensitive to differences in thickness of cover glasses, it is not at all necessary to resort to a mechanical draw-tube if one's instrument is fitted with a carefully made simple draw-tube as is generally provided in the best Continental stands.

It may be mentioned, however, that still better results would be obtained by resorting to the use of a cover-glass gauge, an instrument which is very valuable and inexpensive, but rarely used by workers. In dealing with the standardisation of the microscope, the matter of cover glasses, which is a serious one, should also be considered with the view of providing microscopists with covers of much more uniform thickness.

Mr. Sutcliffe, in his letter which appeared in NATURE of January 18, asks if there is any evidence obtainable that the English instrument has lost its premier position. We have not far to seek the necessary information, and when the facts are known we cannot consider the English instruments "degenerate," as the present models are much superior to their predecessors. The best English microscopes are a credit to any well-organised manufactory. The Germans, however, have advanced further in the direction of providing more perfect instruments, which are produced in works better organised and with more highly scientific arrangements.

The instruments that combine in the highest degree simplicity, efficiency, and durability are the most valuable to the serious workers, and these features are strongest in the best Continental microscopes.

I do not believe any amount of discussion would bring us to a decision so satisfactorily as a close examination of instruments which have been in use for a number of years in the principal laboratories throughout the world. In fact, we need hardly look outside our own little island for the result, as there is plenty of material at home to deal with for a settlement of the dispute in a most practical manner.

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18 Bloomsbury Square, London, W.C., January 30.

The Mnemic Theory of Heredity.

IN a review of the third edition of Prof. Richard Semon's well-known book upon this subject, in NATURE of January 18 (p. 371), the reviewer writes as follows:—"The mnemic theory, which is based upon a belief in the inheritance of acquired characters, naturally does not appeal to those who deny the possibility of such inheritance." From the point of view of modern embryological research, both of these statements are open to challenge. The founder of this mnemic theory, or "memory as a general function of organised matter," has indeed written very little upon the subject, which he first broached in a public lecture in 1870. At the date named, when Prof. Ewald Hering, now of the University of Leipzig, gave his classic address, I imagine that the question of the inheritance or non-inheritance of acquired characters had hardly been raised. By a curious coincidence, it was in the same year that Prof. W. Waldeyer in his researches set up the doctrine of the somatic origin of germ-cells from the "germinal epithelium," and obviously this doctrine of the somatic or bodily origin of germ-cells is demanded by the view of an inheritance of acquired characters. The history of embryological research upon the germ-cells during the present century demonstrates clearly that in his researches in the lower vertebrates the writer first established—in 1900—an actual tangible continuity of germ-cells from generation to generation, and the absence of any genetic connection between Waldeyer's "germinal epithelium" and the germ-cells, that in many other cases these finds have been, and are still being, confirmed by the investigations of other observers, and that Prof. Waldeyer himself some years ago withdrew his former researches in favour of such a continuity of germ-cells as underlying the life-cycle. Indeed, he wrote:—"The consequences of this doctrine of the continuity of germ-cells are almost incalculable for every branch of biology"; so that now for animals a fundamental postulate of the doctrine of an inheritance of acquired characters has vanished.

But, as a fact, the inheritance of acquired characters is more a botanical doctrine than a zoological one. In plants this apparent inheritance of such characters is confined to *the asexual generations*, in which, if the term "individual" be used, it must apply to *all the products*, asexually produced, of one original plant—that is, in this sense they would all form a single individual. As an instance, in this way all the countless plants of the white chrysanthemum, Niveus, would form a single individual. The doctrine of an inheritance of acquired characters does not, and cannot, apply to *the sexual generations of animals*. Moreover, here it is refuted by the positive finds of embryology, which demonstrate, under an actual continuity of germ-cells from generation to generation, that nothing at all is handed on from parent to offspring. As this is the case, nothing acquired by the parent can be inherited. That is, there is in the sexual generations of animals no such thing as an inheritance of acquired characters.

I do not now remember when I first came to know of Prof. Hering's theory of heredity as based in the unconscious memories of germ-cells, but it must be a long time ago. As Samuel Butler remarks of Charles Darwin and the doctrine of natural selection, it must have become an "unconscious memory" to me, for in my researches into the history of the germ-cells I evolved it anew. As showing this, and as affording a simple statement of the main lines of the theory, I may quote the following,[1] written by me in 1904:—"From its nature it (the theory of heredity advocated by me) might be termed 'the Understudy-Theory of Heredity.' Given in a certain life-history the period of formation of the primary germ-cells. Of these let there be, for simplicity, but two, AB and BA. On one of these falls the lot of developing into an embryo: to which of the two this happens is not of consequence for the argument. In all its essential characters the remaining primary germ-cell (whose immediate destiny it is to become the founder of the 'sexual products' of the said embryo) is the exact counterpart of the developing one. So much so is this the case that, if both form embryos, these are identical twins. In the ancestry neither of the primary germ-cells, AB and BA, had ever been a higher animal; neither they nor their ancestors had ever formed part of the body of a higher animal. But their ancestry is continuous with a long line of germ-cells, and at regular intervals these were exactly like certain sister-cells, which did develop and form higher animal individuals. Although the cell AB does not itself give rise to an embryo, in the meantime it retains for itself, and also for all its immediate progeny, the properties of BA, those characters or potentialities which, were it or any of its progeny to develop, would make it or them identical twins with BA, the other cell which did develop. *This is the greatest wonder in embryology!* In the drama of heredity there are always understudies, which for a certain essential period are endowed with all the identical properties (potentialities) of that germ-cell from which the player arises. These understudies, the primary germ-cells, are never employed upon the stage as such—except in the instances of identical twins, triplets, &c.—but some of them in new guises and after new conjugations or unions are the immediate ancestors of those which become the acting characters in new scenes of the cyclical drama of Life."

The original German of Prof. Hering's lecture, like Austrian German in general, is difficult, and probably the original is little known in this country, although an excellent translation was published in Butler's "Unconscious

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[1] Beard, J., "A Morphological Continuity of Germ-Cells as the Basis of Heredity and Variation," in *Review of Neurology and Psychiatry*, vol. ii., 1904, p. 141.

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